

condensed tables for alkaloids and gases, which are, however, in themselves very good ones. It is to be feared that these practical books tend to make students mere analytical machines in a small way, without giving them much real practical notion of chemistry. It is questionable whether a student who has worked through the modern tabular system of practical chemistry would be able, for instance, to state the reason for the employment of bricks in preference to chalk for the back of an ordinary fireplace or some equally simple practical question.

Elementary Chemical Arithmetic. By Sidney Lupton. (London: Macmillan and Co., 1882.)

THIS little book with its modest preface will be recognised by all teachers of chemistry, especially in large laboratory classes, and also by students as a really useful adjunct.

Unfortunately in large public laboratories a considerable proportion of the students have been very much neglected in the matter of their elementary mathematical education, or it has been of such a nature that they are not able to apply it to the solution of ordinary chemical problems, thus entailing, in many cases, a large amount of extra work and loss of time on the part of the teacher in giving instruction in elementary arithmetic. This book fits into its place exactly. It is divided into two main portions: an introduction, consisting of short but very understandable explanations of arithmetical processes in common demand in chemistry and physical chemistry of a practical and elementary nature, the second portion being problems divided under the headings of the different elements. Regarding these it may perhaps be said that they do not err on the side of being too chemical, and in one or two cases more attention has been given to the question as a question than to its absolute chemical correctness, but these are mere details that in no way detract from the utility of the book for its purpose.

What is required of the mass of chemical students is that they should be able to apply methods of reasoning founded on experimental facts in the science to the solution of concrete and abstract problems; and working through this book will certainly conduce to bring about an improvement in that direction.

The Watch and Clockmaker's Handbook. By F. J. Britten. (London: Kent and Co., 1881.)

THIS little book has been written, we are informed, chiefly for the instruction of country watchmakers. It cannot fail to be agreeable to them: it contains a great deal of useful practical information, and some is given of a higher quality, such as workmen are, to their credit, eager for now-a-days. To another and wider circle there is also much of a character to be interesting. The book is a proper supplement to the more popular horological treatises. There are good descriptions and pleasing diagrams of the various watch escapements; there is a chapter upon the art of springing; the mechanism of chronographs, repeating watches, and calendars is shown, but almost too briefly. Lastly, we find pictures and a short reference to the various tools which watchmakers employ, and some serviceable memoranda are added. Upon the whole the author has and deserves our praise.

H. DENT GARDNER

Heroes of Science. Botanists, Zoologists, and Geologists. By Prof. P. Martin Duncan, F.R.S., F.L.S. (London: The Society for Promoting Christian Knowledge, 1882.)

THIS little volume contains brief sketches of the lives of a few botanists, zoologists, and geologists, for the most part acknowledged compilations from well-known sources. No doubt the work will serve the purpose for which it is evidently intended—that of interesting young people in science.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Physics of the Earth's Crust

ON March 23 last Prof. Green sent to NATURE some remarks upon Mr. Hill's review of my "Physics of the Earth's Crust." More lately the third edition of his "Physical Geology" has appeared, in which he has repeated the substance of a part of what he then wrote. On account of the great weight which his authority will carry, I think I should offer some reply.

He truly says at p. 674, that I claim to have proved that the contraction of the earth through cooling cannot have caused the amount of squeezing and elevation which has taken place, and that the hypothesis is therefore insufficient to explain the facts which it professes to account for; but he then adds: "What Mr. Fisher has really done is this. His calculations go far to prove that, provided the earth cooled in the way assumed by Sir Wm. Thomson, contraction would not suffice to produce anything like the compression and elevation that has actually occurred. But this is quite another thing from disproving the contraction hypothesis. Mr. Fisher's investigations tend rather to establish a strong probability that the earth did not cool in the way supposed by Sir Wm. Thomson,"—that is, that it became solid throughout in a comparatively short space of time. But of course my calculations do not establish any probability against this way of cooling, unless we begin by assuming that contraction through cooling has been the cause of the elevations. And that seems to be begging the question. What they do prove is that the contraction hypothesis will not account for the elevations if the earth has cooled as a solid.

But there may have been another way of cooling which, on geological grounds, I believe to have been the true one. The earth may not have become solid throughout in a short space of time, and may not be solid even now. In that case the crust, whose corrugations we have to account for, must have floated on a denser liquid substratum. Under these circumstances every elevation above the mean level must have had a corresponding protuberance answering to it below. This is necessary, as was long ago pointed out by Sir G. B. Airy. I have, then, proved that, this being so, if the crust beneath the ocean is of the same density as beneath the continents, on what I conceive to be reasonable assumptions regarding the thickness and density of the crust and the density of the substratum, a shortening of the earth's radius by less than 700 miles would not have sufficed to produce the existing inequalities. I can imagine no theory of the constitution of the interior that would admit of so large an amount of contraction taking place, after the whole had become sufficiently cool for a crust to have begun to be formed, as to cause such an amount of shortening as this.

If, however, we suppose that the crust beneath the oceans is denser than that which forms the continents (and I have given several reasons for believing such to be the case), then a much smaller amount of radial shortening would suffice. I have estimated it at about forty-two miles. Still, anything near this shortening is far beyond what any reasonable amount of contraction from cooling could produce. For if there be a liquid substratum this must be of nearly equable temperature throughout, and that cannot be much above the temperature of solidification; so that it does not appear how a much greater contraction can be got out of the gradual solidification, and incorporation of the upper parts of the liquid layer with the crust, than could be obtained on the former supposition of a cooling solid globe; and I have shown that, in that case, the radial shortening would be less than two miles.

Thus, then, I claim to have disproved the contraction-hypothesis under the two alternative hypotheses (1) of a solid globe, and (2) of a liquid substratum.

Capt. Dutton, of the United States Geological Survey, has said of this part of my work, "First and foremost he has rendered most effectual service in utterly destroying the hypothesis, which attributes the deformations of the strata and earth's crust to interior contraction by secular cooling. No person, it seems to me, can sufficiently master the cardinal points of his

analysis, without being convinced that this hypothesis is nothing but a delusion and a snare, and that the quicker it is thrown aside and abandoned the better it will be for geological science" (*American Journal of Science*, vol. xxiii. p. 287).

I take this opportunity of pointing out a mistake in my book. At page 156 the number 1127 ought to be 1734; and consequently the number 0.996 ought to be 0.965. The argument will still hold.

O. FISHER

Harlton, Cambridge, November 9

P.S.—Since forwarding the above I have observed a note at p. 912 of Dr. Geikie's "Text Book of Geology," in which he says that I have "endeavoured" to show that the secular contraction of a solid globe through mere cooling will not account for the phenomena. The word "endeavoured," does not express the attitude of my mind upon the question. Forty-two years ago the contraction theory occurred to myself independently. I remember that in my youthful joy at what I thought thought a discovery, I forthwith vaulted over a gate! In 1868 I read my paper on "The Elevation of Mountains by lateral Pressure," fully believing that I was elucidating the cause which had produced them in the contraction through secular cooling. In 1873 I began my paper on "The Inequalities of the earth's Surface viewed in connection with the Secular Cooling," while still under the same impression. I first of all estimated the actual elevations, and, this done, I calculated the amount of those which would be formed upon Sir William Thomson's view of the mode of solidification. To my excessive surprise, the result showed the utter inadequacy of the contraction hypothesis. I thought I must have made some error in the calculations, but could find none. I still, however, adhered to the original idea of contraction, and suggested, towards the end of that paper, a fluid condition of the interior at some former period, thinking that sufficient contraction might be perhaps obtained by that means; for I had not yet dared to question Sir Wm. Thomson's dictum of the present complete solidity of the earth. It was not until about a year ago, when I wrote the chapter in my book about the "Amount of Compression," that I perceived that even the condition of a liquid substratum would not give the necessary degree of contraction to produce the compression. I have thus been driven from the contraction hypothesis step by step, and have by no means been endeavouring to support a preconceived opinion against it.—O. F.

Shadows after Sunset

HAPPENING by chance to look into "Loomis's Meteorology," after reading M. Dechevren's account of the blue, white, and red bands visible before sunrise and after sunset at Zikawei, I noticed under the above heading the following account of shadow-bands, which not only appear to be very similar to those observed by Dechevrens, but are explained in identically the same way ("Loomis's Meteorology," p. 107): "A similar phenomenon [to the water-bands described in the preceding paragraph] is frequently noticed about fifteen minutes after sunset, when the shadows of clouds near the horizon are projected upon the western sky in the form of radiant beams diverging from the sun. These beams are parallel lines of indefinite length, but from the effect of perspective they seem to diverge from the sun, and if they could be traced entirely across the sky, they would for the same reason converge to a point directly opposite to the sun. Such cases are sometimes, though not very frequently noticed. Similar shadows are sometimes seen in the morning before sunrise, and form a conspicuous feature of the morning twilight. This effect is sometimes noticed in nearly every part of the world. It must have attracted the attention of the ancient Greeks, and is thought to explain that poetic expression "the rosy-fingered dawn."

M. Dechevrens appears to think the phenomenon does not occur in Europe or temperate latitudes generally, but from what Loomis says, one would infer that he may be mistaken in this, and that to a modified extent it may be visible in Europe and America. Perhaps some of your readers who are in the habit of observing the face of the sky will be able to verify this supposition. For my own part I have not remarked it in England, but have occasionally witnessed it in Bengal during the rains, very markedly. The explanation offered by M. Dechevrens seems the only reasonable one under the circumstances, but he hardly seems to lay sufficient stress upon the fact that when the sun is below the horizon his rays can only illuminate a shallow

stratum of partially condensed vapour in the upper atmosphere. Any obstruction of his rays will consequently shut off the whole of the reflected light from this stratum, and cause the blue sky to appear through the shadow, all the more cerulean by contact with the whitish or rosy colour of the adjacent portions which still bask in the solar rays.

E. DOUGLAS ARCHIBALD

An Abnormal Fruit of *Opuntia Ficus-Indica*

THE accompanying figure represents a fruit of *Opuntia Ficus-Indica*, which is wholly inclosed in one of the well-known flat branches of this plant; normally the fruits appear as exserted obovate bodies on the margin, or on either side, of the branches. The figure is exactly half natural size; the fruit is therefore full grown. There is no interruption in the ascending curves of spinous tubercles, only they are somewhat smaller on the fruit, which has also a less wrinkled skin than the remainder of the branch. It is of rather uncommon occurrence, nobody having seen here anything alike in the extensive *tunales* or Indian fig-plantations of our neighbourhood; nor have I been able to find any mention of such a case in the books at my disposal. It is evidently an instance of non-development of peduncle, a special case of suppression of axile organs (Masters, "Teratology," p. 393). But I think it throws also some light on the nature of what generally is taken to be the pericarp of the *Opuntia* fruit, which, after all, seems to be a slightly modified branch, bearing the ovary of the flower in a cavity on its



Abnormal Fruit of *Opuntia Ficus-Indica* from Caracas.

upper end. A similar view is held forth by Dr. Noll in a paper published in the *Annual Report* of the Senkenbergische Gesellschaft (Frankfurt, 1872, pp. 118-121, with two plates), where he describes and figures two abnormal fruits of *Opuntia coccinellifera* from the Canary Islands, with branches growing from the exterior part of the fruits. Their apparent pericarp is therefore an axile organ of a certain order, say of the order n , whilst the additional branch is of the next order, $n+1$. The case which forms the object of the present note is quite the reverse of those mentioned by Dr. Noll, as the branch of order n , or the exterior part of the normal fruit, is not developed independently, being represented by its parent-branch of order, $n-1$.

If this view be correct, there can no longer be any reason for speaking of an exserted ovary in *Opuntia* (Hooker and Bentham, "Genera plantarum," I., 851), as this organ is wholly sunk in the interior of a branch, just as it happens in other *Cactææ* with an ovarium immersum.

A. ERNST

Caracas, October 4